Review of the revised version of the paper « Comparing model and measured ice crystal concentration in orographic clouds during the INUPIAQ campaign» by R.J. Farrington et al. submitted to ACP

In the initial review of the paper, my main questions concerned the reliability of a surface hoar flux emitted from the surface and the potential influence of blowing snow. With this new version of the paper, the authors made an effort to clarify their formulation of the surface flux and discuss the differences between their approach and the approach followed by the community modelling blowing snow (e.g. Gallée et al., 2001; Lehinng et al., 2008; Vionnet et al; 2014). Prior to publication the authors should improve the organization of Section 3.4 and modify some points of the discussion concerning their formulation of the surface flux.

Overall I am now satisfied with this new version of the paper. I still believe that future work combining modelling and observations is required to better quantify the importance of surface flux of ice crystals (both blowing snow and surface hoar) to explain high ice concentrations in orographic clouds. It could be done through interesting collaborations between the "cloud" and "snowpack" scientific communities.

With the revision, the size of Section 3.4 has increased and the current version is not easy to follow. The authors should consider rewriting this section. They could present first their formulation of the flux, then the results (Fig 12 to 14) and finally discussed the results (comparison with formulations used when modelling blowing snow in the atmosphere, impact of the assumption concerning the size of emitted crystals, relationship between wind speed and modelled concentration (Fig 15)). Doing this, the clarity of this section would be improved.

The authors has modified the conditions under which the flux of ice crystals is emitted towards the surface accounting for: (i) a positive latent heat flux toward the surface to represent conditions in favour of surface hoar formation and (ii) a threshold wind speed for the removal of surface hoar by wind set to 4 m s^{-1}. The restrictions applied to the flux are described at P. 25 (l 523-538). The authors also mention conditions on air temperature and relative humidity. Overall, are all these conditions applied in the new simulations done for the revised version of this paper? It is not really clear when reading the paper.
It could be also mentioned that conditions (i) and (ii) are somehow opposite. Indeed, condition (i) concerns periods in favour of surface hoar formation whereas condition (ii) concerns periods of crystals removal by the wind.

The authors discuss at P. 24 (l. 491-504) the similarities and differences between their formulation of the flux and the formulation used when modelling blowing snow in the atmosphere, especially the formulation employed in Vionnet et al. (2014). Some points of this discussion should be modified:

(1) The remark made at l. 501-503 by the authors concerning the supposed similar exponential relationship between Xu et al (2013) and Vionnet et al (2014) is erroneous. Indeed they try to compare the flux of Xu et al (2013) which is a vertical flux from the surface toward the atmosphere while fluxes on Fig. 8 a of Vionnet et al (2014) are horizontal fluxes at different heights above the surface.

(2) The two formulations do not only differ because of the turbulent terms. Indeed the formulation of the vertical flux of blowing snow emitted towards the atmosphere is different (see Eq. 17 of Vionnet et al. 2014).

At P. 27 l 575-579, the authors mention the importance of the assumption concerning the size of crystals emitted towards the atmosphere. The sentence at l 579-580 suggests that increasing the size of the crystals would improve the match between modelled and observed IWC. On the other size, it will lead to faster sedimentation. This limitation should be explicitly mentioned here.

Specific comments

P. 8, l 220 : the authors mentions that model outputs are taken at the 1st atmospheric level. Is it the 1st prognostic level or the diagnostic level (typically at 2m for T and ReHu and 10m for wind in atmospheric models)? At which height above the ground is located this level? The authors should mention it since it may influence model evaluation.